We would like to thank the reviewer for his/her feedback on this study, which helped us clarifying some points and improving our paper. Our answers to the different comments are detailed below.

Remarks from the reviewer are in blue while our answers are in black. The changes proposed for the revised manuscript are in italic (*grey* for unchanged sentences and ***bold black*** for modified/new sentences).

Reply to Referee ♯2 : Specific comments

1. P.7, l.22. It is worth to mention that 4D-Var assumes the model is perfect. Problems can be expected if model biases are large.

Do you expect that your results would significant change if the authors use other model besides MOCAGE ? And why ?

The specification is given in the revised manuscript:

The assimilation configuration used for this study is based on the 4D-Var (4-Dimensional-Variational) algorithm ***in a "perfect model" framework.***

Concerning the following comment of the reviewer:

We agree about the fact that a strongly biased model could raise issues within a strong constraint 4D-Var framework. However, for our particular application, we think that this is not a major issue. In the original manuscript we always showed results from either the direct model (DM) or the reanalyses, but omitted results from the 4D-Var forecast cycles (12 hours-long). The large biases of the DM (e.g. Figure 3 of the manuscript) can trick the reader about the fact that these biases could affect the forecast step of the 4D-Var as well. However, after few cycles of data assimilation, the global ozone field is already strongly corrected by the dense observation network, and the forecasts used as background for the assimilation are significantly less biased than the DM (figure 1). The fundamental reasons are the relatively long life-time of free tropospheric ozone and the fact that we employ a linearized chemistry scheme. Please refer also to [Emili et al., 2014] for a more detailed discussion about this aspect.

We conclude that, except for the very initial period of the assimilation experiments (usually some days, see Fig. 10 of [Emili et al., 2014]), forecast biases do not represent a major issue in our study.

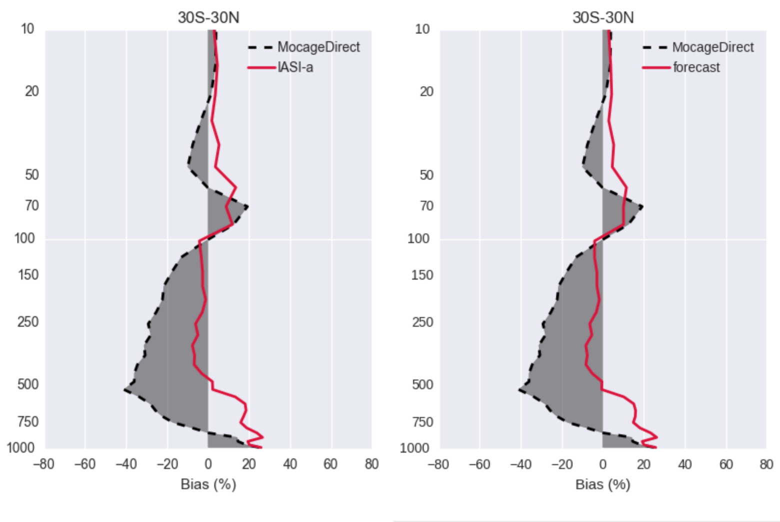

Figure 1: Validation for November-December 2008 of direct model (dotted black lines), IASI-a (left) and the IASI-a forecast (right) versus ozonesondes. Validation has been done between 30° S and 30° N.

Concerning the question about the possible results using other models than MOCAGE or even a different chemistry scheme within MOCAGE itself:

The constraint provided by IASI dense observations on modeled O_3 is quite strong in the free troposphere and UTLS regions (e.g. figure 3 of the manuscript). We presume that these results would remain similar using other chemistry or meteorological schemes with different O_3 prediction skills. However, the biases in the planetary boundary layer (PBL) due to the limitations of the employed chemistry scheme (CARIOLLE) cannot be corrected by IASI assimilation. Hence, it would be interesting to use a chemistry transport model that is more accurate in the PBL. The resulting improved TCO columns could further reduce the residual bias that we found for our reanalysis compared to OMI-MLS estimations (figure 7 of the original manuscript).

We have mentioned in our perspectives to use "a more comprehensive chemical scheme that accounts for the surface emissions. The computational cost of these simulations will be however much larger".

2. P.8, l.6. 'We specify the background error variances as a percentage of the modeled ozone profile equal to 15% in the troposphere and 5% in the stratosphere.' How sensitive of your results to this setting ? Since 4D-Var might be sensitive to the initial condition. I think it would be important to address this issue.

The specification of the background error covariance matrix is an important issue in variational data assimilation. However, a detailed sensitivity analysis cannot be easily conducted for such long reanalyses due to the cost of the simulations. Therefore, the choices being made for this study are mostly based on previous studies, with some additional but minor tuning being made based on sensitivity analyses for a two-months long simulation (e.g. result at line 10, page 8 of the original manuscript).

Concerning the background error variance in particular, previous studies using the same model configuration as us, compared different options: using a simple percentage of the background field (15% [Massart et al., 2012]) or estimating it from ensembles of simulation plus online statistical optimization based on misfit values [Massart et al., 2009, Massart et al., 2012]. The different standard deviation estimations did not influence significantly the analysis quality [Massart et al., 2012].

The choice of a percent background variance that depends on the height is discussed in [Emili et al., 2014], which represents the main reference concerning the assimilation set-up of our manuscript. A sensitivity analysis of the B parametrization is discussed in section 4.3.1 of [Emili et al., 2014], who also did not found significant differences among different specifications of the variance. Focusing on the tropics, we found slightly more accurate results against ozonesondes using a background error standard deviation of 15% in the troposphere instead of 30% as in [Emili et al., 2014]. Therefore, we kept this value for the long reanalysis.

We added the following lines in the revised paper in order to better clarify:

Most of the parameters of the assimilation algorithm used to compute the reanalyses in this study are based on the study of [Emili et al., 2014]. ***The validation of a short reanalysis of two months against ozonesondes (not shown) has been used to further optimize some of these parameters****. The background and observation errors are defined as follows.* ***[Emili et al., 2014] have assimilated IASI and MLS data globally with a background error standard deviation equal to 30 % of the modeled ozone profile in the troposphere and 5 % in the stratosphere. Based on local validation in the tropics, we found slightly superior results using a value of 15% instead of 30% in the troposphere. Therefore, this choice has been taken for the 6-years reanalyses.***

3. P.8, l.21. Why second and third simulations don't require spin up ?

We have used a climatological field on 1st November 2007 to initialize the model and to allow a spin-up period of two months. The initial condition for the second and third simulations (MLS-a and IASI-a) is the direct model output on 1st January 2008. Consequently, we have used the same spin-up period as in the direct model simulation.

We clarify the spin-up period for MLS-a and IASI-a in the revised paper:

The model is initialized with a climatology on 1st November 2007 to allow for a spin-up period of two months. The second simulation, named MLS-a, started in Jannuary 2008 with the assimilation of MLS profiles for the whole period. Finally, the third simulation (IASI-a) was produced with the assimilation of the IASI tropospheric O_3 columns and the MLS stratospheric O_3 profiles. ***Both MLS-a and IASI-a are initialized with the direct model output on 1st January 2008.***

4. P.9, l.10. Please specify which colocation method you apply here ?

We included the following explanation to the revised manuscript:

O_3 data have been first treated as follows : i) The modelled fields have been collocated with the soundings in space and time, ii) The obtained values have been averaged on a two-months basis, in order to take into account a larger number of soundings for statistical evaluations. ***The collocation was done with a linear interpolation of the model's 6-hourly outputs on both the horizontal/vertical dimensions and in time.***

Bibliography

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